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Criticisms of the specific theory
of hydrophobia

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CRITICISMS ON THE SPECIFIC THEORY OF HYDROPHOBIA.

By

CHARLES W. DULLES, M.D.,

SURGICAL REGISTRAR TO THE HOSPITAL OF THE UNIVERSITY OF
PENNSYLVANIA.

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NEARLY two years ago I had the painful experience of being summoned one day to take charge of a boy who was suffering with what is called hydrophobia. The unfortunate patient was under my care and observation less than twelve hours, when he died. The horrors of that day can only be understood by those who have had a similar experience. To stand in the presence of such distressing psychological disturbance, to see death advancing relentlessly, to feel that science furnishes no satisfactory explanation of the nature of the disorder and offers no trustworthy remedy for its treatment—this was a trial of mind which intensified the weariness of body which the circumstances necessarily entailed. The details of the case were read before the West Philadelphia Medical Society, and are recorded in the *Medical News*, May 27, 1882. There can be found all the points which at the time most impressed me. There, too, are recorded the theoretical and practical notions in regard to hydrophobia which I had at that time.



Later, on May 23, 1883, I read before the Philadelphia County Medical Society a paper entitled "Remarks on Hydrophobia," which was published in the *Philadelphia Med. Times*, Aug. 11, 1883. In this are set down some of the conclusions which I had reached after more than a year's study of the literature of the subject. The point aimed at in that communication was to attract the attention of those who heard me to the extremely unsatisfactory condition of our knowledge in regard to this disorder, and to lodge in their minds the suspicion that the views so ordinarily held, and so positively stated in most of the text-books, are not as clearly established as most people believe. In concluding the discussion which followed in the Society, I stated all I wished to, at that time, of my own convictions, in the expression that "but little of the evidence upon which it (the commonly accepted theory of hydrophobia) rests, can be regarded as having *an accuracy and reliability commensurate with the delicateness and gravity of the questions at issue.*" Since then I have continued my investigations, and the wider their extent has grown the firmer has become my belief that this statement was the very mildest that could have been used without sacrificing the truth. But, I have learned by reading, and by experience, that those who differ from the majority are in danger of being treated to a certain amount of disdain for their inability to accept as evidence what to others seems acceptable enough. For example, Dr. Parry, in his interesting work on *Cases of Tetanus and Rabies Contagiosa*, published in London, 1814, indicates his sentiments as follows: "It was formerly doubted whether any such disease

as Canine Hydrophobia existed;" and I have heard that the question was once solemnly debated in a society, which, after a long discussion, determined it in the negative by a majority of voices. The existence of the Giraffa, or Camelopardalis, was long denied by the moderns, not only notwithstanding the accounts given of it by the ancients, and its delineation on the Prænestine pavement, but after its skin was actually to be seen dried and stuffed in a Dutch museum. We are told of a king of Siam, who, after he had heard with patient attention certain wonders from a European ambassador, and was at last informed by him, that in his country, at certain seasons of the year, water became solid, and was capable, not only of being cut or broken, but of admitting his Majesty and his whole court to walk over it without wetting their feet, flew into a violent passion, exclaiming, "For a long while, notwithstanding the prodigies you have related to me, I thought you were an honest man; but now I am sure that you are an impudent liar." In this story, the intimation that our knowledge about hydrophobia has reached the "dried-and-stuffed" stage goes well with the other intimation, that the skeptics in regard to it are in the state of ignorance comparable to that of the king of Siam.

Later than Dr. Parry, but still in this century, Dr. Copland set the stamp of his learning on the doubters thus: "The supposition lately published that there is no such specific disease as rabies, and that it is merely the result of mental anxiety, etc., is only one of the absurdities thrown up on the surface of medi-

cal doctrine, and hardly deserves mention, and much less serious refutation."

But scientific questions are not settled in this summary way, nor has the fear of being thought singular ever long deterred men from asserting what they had come to hold as convictions. That which has been branded as an absurdity has, over and over again in the history of the world, come to be established as a fact, and the same may yet be accomplished for the opinion of those who deny the specific nature of rabies or hydrophobia. One thing is sure, viz., that the attitude of severe criticism seems to offer the only hope for the solution of the riddle of hydrophobia; since, in more than two thousand years, the acceptance and repetition of authoritative opinions has failed to furnish a theory which does not fly in the face of all analogy, or a practice which holds out a reasonable hope of saving a single life. Such is the real state of knowledge in regard to hydrophobia. The theories as to its nature are contradictory and illogical; the teachings as to its treatment are without the slightest hope of success. In regard to both, the ablest men who have discussed it occasionally or systematically are in irreconcilable opposition. While some hold to certain modes of communication, others strenuously deny them. While some admit its spontaneous origin, others will not admit it for a moment. While some think the virus is to be found in the saliva, the blood, the bronchial mucus, the milk, the flesh of rabid animals, others limit it to the first of these alone. In regard to the period of incubation the widest divergence of opinion exists. The range is from one day to forty years, and no one seems to

have been able to fix a limit to either its shortness or its length. Equal contradiction meets the gaze when one turns to the matter of treatment. From the throwing into a fish-pond, as practised by Celsus, to the inhalation of oxygen, as recently practised by Drs. Schmidt and Zebeden, there is a variety of methods which demonstrates how utterly every attempt to find a specific has lacked the guidance of any accurate idea as to the nature of the disorder. To almost any statement of men whose names are highly respected in medicine, may be opposed the most positive and contradictory statements of others equally respected. Virchow admits (*Handb. d. Spec. Path. und Therap.* “*Wuthkrankheit und Wasserschew*”) that hydrophobia may be communicated by means of weapons used to kill mad dogs with, and by blood-letting and scarifying instruments—a belief which, but for Virchow’s adherence to it, would be correctly designated as obsolete, and which is entirely unworthy of acceptance. Bollinger, a distinguished veterinarian, admits (Ziemssen’s *Cyclop.*, art. “Hydrophobia”) the possibility of infection by the migrations of animal parasites, such as fleas and lice.¹

The late Dr. George B. Wood admitted the possibility of hydrophobia being caused by the bite of birds, and explains why this does not often occur as owing to the fact that these animals, like horses,

¹ This curious idea is founded upon a reference, which, when I hunted it up, I found to be a pure assumption of a Dr. M’Crae (*Lancet*, March 23, 1872, p. 420) that the occurrence of smallpox thirteen miles northeast from Melbourne, where it was also present, was to be accounted for only on the supposition that it was conveyed by flies. It has been interesting to observe, in my own investigations, how this supposition of Dr. M’Crae’s has grown to a statement of an established fact, as it has been quoted by foreign writers.

asses, and oxen, do not usually bite. (*Practice of Medicine*, vol. ii. art. "Hydrophobia.") In view of these facts it will not seem so remarkable that Sauvages believed the eyes of hydrophobic patients to shine at night like those of cats, or that he should explain this on "meaque de hydrophobia theoria, quæ admittit in hoc morbo fluidum illud in phosphoricam activitatem exaltatum." (*Nosologia Methodica: Vesaniæ*, vol. ii. p. 232.) Nor can one wonder that Mead, about a hundred years ago, should have said: "The influence of the moon in these cases, I am convinced, is of some weight." (*Medical Works*, art. "Hydrophobia," p. 62.)

But, to return to modern times, what shall one think of the attempt of Doléris, the author of the, in many respects, admirable article on this subject in the *Nouveau Dictionnaire de Médecine et de Chirurgie* (1881), to explain the assertion of the occurrence of spontaneous cases by saying that the disorder may be acquired from germs distributed in the air and earth, like "charbon?" Or, how shall we consider the willingness of Sir Thomas Watson (*Nineteenth Century Review*, Dec. 1877) to accept the occurrence of hydrophobia by the infection of sucklings through the medium of the mother's milk, or his statement that the disorder does not occur in Constantinople or Africa? In both which places it does occur.

But I do not wish to make too much of this aspect of the literature of hydrophobia. I wish to call your attention to the cause of the uncertainties which prevail in regard to it. This is, in my opinion, easy to point out. It is the too great readiness to accept any

sort of testimony which may be offered as bearing upon the subject. This fault is to be found in almost all the works treating of hydrophobia, from the earliest ages to the present time. This led Pliny to father such absurdities as the following: "There is a small worm in a dog's tongue, known as 'lytta' to the Greeks; if this is removed from the animal while a pup, it will never become mad or lose its appetite. This worm, after being carried thrice round a fire, is given to persons who have been bitten by a mad dog to prevent them from becoming mad. This madness, too, is prevented by eating a cock's brains; but the virtue of these brains lasts for one year only, and no more." And, again, "So virulent is the poison of the mad dog, that its very urine even, if trod upon, is injurious, more particularly if the person has any ulcerous sores about him. The proper remedy in such a case is to apply horse-dung, sprinkled with vinegar, and warmed in a fig." (Pliny, *Natural History*, book xxix. chap. 32. Trans. by Bostock and Riley.) This same too easy acceptance of testimony led such a writer as Dr. Samuel Bardsley, in 1807, to accept the statement that hydrophobia could be communicated by the mere application of the saliva to the unbroken skin, and to cite with apparent approval a story of a man who had hydrophobia from kissing a mad dog previous to its being hanged. (Samuel Argent Bardsley, *Med. Reports*, London, 1807.) And, most recently, it has led Bollinger, already referred to, to attribute the disorder to drinking the milk of an affected animal, and to the act of coitus. (*Ziemssen's Cyclopaedia*, 1875.) While Mr. Williams, in his valuable work on *Veterinary Medicine*, 3d ed. 1882, says rabies can be com-

municated through a "thin epidermis without wound or abrasion."

These citations illustrate the most remarkable feature of the literature of hydrophobia. It would seem as if the mind, when once it has accepted a theory which contradicts all experience in other and fairly well-understood diseases, loses the ability to distinguish good evidence from bad. A striking illustration of this is furnished by Dr. Dolan in his otherwise capital book on *Hydrophobia or Rabies*. In his second chapter this writer calls attention in detail to the rules laid down by Dr. Abercrombie in his *Inquiries concerning the Intellectual Powers and the Investigations of Truth*. These warn against the following fallacies: 1. Receiving as facts statements which are not facts, but opinions. 2. Receiving as facts statements which only assume the relation of facts. 3. Receiving as facts general statements, or the generalization of facts. Dr. Dolan then says: "Some writers, for instance, have maintained with much confidence that a particular state of rigidity of some of the limbs is distinctly characteristic of *ramollissement* of the brain. But further observation has shown that the disease may exist without this symptom, and that this condition of the limbs may appear in connection with other diseases. This observation of facts was in so far correct that this state of limbs does very often accompany *ramollissement* of the brain; the error consisted in giving it as a general fact, or a fact applicable to all cases of *ramollissement*, which is without foundation. Yet such statements, when brought forward with confidence, are often received as facts and

rested upon as established principles; and then the facts by which their fallacy might be detected are apt to be overlooked or forgotten." To this reference to the commonest sources of fallacy Dr. Dolan adds: "We are arming our readers against ourselves." And yet he goes on, apparently with his eyes wide open, to reproduce the fault he has just pointed out in speaking of *ramollissement*, accepting statements which ought not to be entertained for a moment in the discussion of so grave and delicate a subject, and admitting as genuine cases which are transparently spurious. I will give an example of this latter mistake, which illustrates several points at once. Dr. Dolan gives, in his collection of cases, and, after sifting his material, stamps as genuine, the following from Romberg's *Diseases of the Nervous System*.

"CASE LXXIV.—Frederick L., male, æt. 6, bitten on the second finger of the left hand by a dog which had already bitten several other children. Result and time of attack, three months; fatal third day.

"*Treatment*.—A few of the dog's hairs had been cut off to place upon the wound, which in eight days was completely healed. The treatment consisted in taking twelve ounces of blood from the arm, scarifying, and applying cantharides ointment to the cicatrix, rubbing in a scruple of mercurial ointment into the inner surface of the left arm. Dr. Horn also visited the boy, and described his features as expressive of extreme anxiety, while his eyes told a tale of immeasurable misery. He implored that he might not be touched or bled again, as he desired nothing more than to be allowed perfect rest." The post-mortem was made on the 4th Sept. 1820, twenty-five hours after death. The smell of putrefaction was already developed. The muscles were dark red. The lungs were charged with blood. The larynx, the trachea,

and œsophagus were not abnormal in appearance. But, the record says, "the redness of the heart was remarkable, the arteries and veins on its surface looking as if they had been injected. The mitral and aortic valves presented a scarlet hue, the trabeculæ carneæ were darker than usual, the internal surface of the aorta was of a bright red hue as far as the arch, the blood contained in the vessels was dark and fluid, the inner surface of the stomach was as pale as that of the œsophagus. No morbid change was found in any other abdominal organ. At the urgent request of the parents the head was not examined."

The history of this case, as originally described in Romberg's book (*Nervous Diseases*, translation, London, 1853), taken together with the post-mortem appearances, seems to me to show it to have been one of acute endocarditis, probably septicæmic. But no such idea seems to have entered the heads of the physicians who attended it. As a case of hydrophobia they went most heroically to work to treat it.

Picture to yourselves the scene. Think of that little child, only six years old, tested with the useless and dangerous tests of handwashing, and mirror, and with urine sprinkled on his skin—the former showing nothing, the latter causing a paroxysm. Think of his being bled, the cicatrix being scarified and blistered with cantharides, and mercurial ointment being rubbed into his arm. Then fancy him, when the doctors came at him after an interval, imploring only to be left in peace, whilst they, with grim determination, bled him again and dosed him with calomel, after which—to quote Romberg literally—"death came to his relief."

In reporting this as a case of hydrophobia, Dr.

Dolan has followed the opinion of Romberg, who, when he recorded it, had *never before seen a case*. But it strikes me as a very piteous illustration of the truth of what White said (*Doubts of Hydrophobia*, London, 1826) of the course of many cases. He described the usual alarm, the fear of the patient, the suspicion of the doctor, the test with water, the melancholy conviction of doctor and patient, adding: "the patient and himself, therefore, will soon *make out a case*—the one dies, and the other publishes an account of his end." Does it seem unfair to apply such an expression to such a case as has just been cited? On the contrary, does not the history of the case indicate that general eminence in medical science gives no guarantee of infallibility when this vexed subject is approached?

I have selected an illustration of the point I wished to make which should include two names well known and justly distinguished, because their very excellences add force to the proof they furnish that the prepossession of a theory which at the outset demands a certain surrender of the judgment will impair the freedom of the judgment at every subsequent step. Such, it seems to me, is the condition in which every believer in the specific nature of hydrophobia stands. The assumption of a specific virus which, and which alone, is the originator of the disease in any individual compels its adherents to bend their judgment to accept the belief that there is a virus which behaves in a way contradictory to that of every other virus about which we have any positive knowledge. As Lorinser has said (*Wien Med. Wochenschrift*, 1874), "We know very well the ap-

pearances produced, for example, by the bite of poisonous serpents, or by the poisoning of wounds with the poison of glanders, anthrax, syphilis, or cadaveric poison. There are, in general, always the manifestations of more or less rapidly developed inflammation, and of further distribution of the poison through the lymphatics, veins, and lymphatic glands, which organs then, in like manner, manifest similar inflammatory symptoms. Nothing of the kind do we see in wounds made by the bites of mad dogs," etc. And, again: "We know, in all pathology, not a single infective disease which, in regard to the time and symptoms of development, bears the slightest resemblance to hydrophobia."

One might suppose that such a contradiction would lead observers to be constantly on the look-out to find some other way of explaining the phenomena of hydrophobia, and that everything that was presented as evidence in support of it would be subjected to the most rigid scrutiny. But the literature of the subject shows that the very opposite is the case. The *post hoc propter hoc* argument is the one which rules here. A careful perusal of a large number of recorded cases leaves the impression that they have not been studied with any idea that they might be something else than what they appeared to be. Though it is no secret that the dread of water is a symptom of a great variety of affections, one finds over and over again the ablest men resting a diagnosis on this which is acknowledged by most systematic writers to be the least reliable of its signs. Another thing which has struck me in examining the records of cases is the readiness with which reporters, aware of the objec-

tion, so often raised, of the effect of mental influence, accept the statements of friends or the silence of patients in regard to it as evidence that it does not exist. I will give an example of this. In the *Berliner Klin. Wochenschrift*, September 15, 1879, Dr. Findeisen reports a case of a man who died of hydrophobia, and who is positively stated not to have known that the dog that bit him was mad, or to suspect that he had hydrophobia, or connect his disease with the bite. Yet the record states that, on the second day of his disorder, he cried out that he had been bitten by the "poisonous (*giftiger*) dog," and was not crazy.

Another curious thing, to be observed in the literature of hydrophobia, is the way in which the madness of the dog is often—one might say usually—deduced from the diagnosis of hydrophobia in the man; instead of the diagnosis of hydrophobia being made to rest upon reliable evidence that the dog was really mad. It is uncommon to find any proof whatever that a dog to which hydrophobia is attributed was rabid, this being, with rare exceptions, a pure assumption. The one phenomenon of a dread of water usually determines the conviction of the physician that he has a case of hydrophobia before him, and settles his opinion of any dog that may ever have bitten the patient, and cannot now be accounted for. Nay, more, this mode of reasoning has led certain writers to the assertion that there may be a passing state in dogs in which they may communicate to a human being hydrophobia, but themselves recover. Thus is the struggle to support the specific theory kept up when circumstances seem to be most adverse to it.

Once more, and I am done for this occasion with

objections to the anomalies which one meets in examining the basis upon which the general belief in regard to hydrophobia rests. I find in the recorded cases—with very rare exceptions—that, when once hydrophobia is suspected, every phenomenon that occurs during life, every lesion that is found post-mortem, is unhesitatingly set down as a feature of this disorder; and the idea does not seem to be entertained that many of them are grounds of suspicion that some other disorder is present. In this way all lines of distinction between hydrophobia and disorders which simulate it are erased; and it remains for every medical man who is called to a patient who has a dread of water, accompanied with exaltation of reflex excitability, to inquire for a bite or scratch of a dog or cat; if one cannot be drawn out, to assume that it has been received and forgotten; then to make a diagnosis of hydrophobia; after which it is only necessary for the patient to die to make a complete record. If to this be added a careful microscopical examination of the state of the brain and spinal cord, the observer will have an exact counterpart of the process by which the famous conclusions of certain investigators—often quoted by those who have not taken the pains to go to the evidence upon which they are founded—were arrived at.

This suggests a brief consideration of the pathological lesions of hydrophobia. Notwithstanding all the diligent endeavors made to learn something from the post-mortem examination of the bodies of dogs and men that have died of rabies or hydrophobia, one might to-day adopt the language of Dr. Hart, who declares (*Cooper's Surgical Dictionary*, art.

"Hydrophobia") that these examinations afford "little else than a varied mass of symptoms, common to other nervous and blood disorders," and fail to "throw any clear light on the pathology of the disease."

I will not trouble you with the disappointed anticipations of the ancients in this connection; those of modern times are more instructive. The announcement by Hallier, in 1872, of the discovery of a micrococcus which was the germ of hydrophobia, and to which he gave the positive name of "lyssophyton," and the belief of Klebs in the parasitic nature of the granular elements found around the bloodvessels near the scar, have appeared and disappeared within the last decade. The hasty statement of Pasteur that he had obtained from the saliva of a patient dying of rabies a peculiar germ which, when he had cultivated it and inoculated the product, gave rise to a hitherto unknown disease, he was compelled to withdraw almost as soon as he had made it.¹

The investigations of Benedikt, Coats, and Gowers are of the greatest interest in connection with this part of our subject. They are often spoken of as if their results were identical and conclusive; although, in fact, they are neither. Dr. Benedikt published his investigations in 1875, in *Virchow's Archiv*. The text is accompanied with a plate which leaves little room for misunderstanding the author's meaning. The

¹ Nothing daunted by the experience of others, I observe that Dr. Paul Gibier reported to the Acad. des Sciences, of Paris, on the 11th of June, 1883, that he had found a special organism in the rachidian fluid of animals that had died of rabies, and that the virus could be attenuated by means of cold. Another of the conclusions he announced at that time was that the disease is communicable from mother to fetus.

lesions described and figured consist of a perivascular exudation or extravasation (or both) of *red* blood-corpuscles, miliary spots of finely granular degeneration of the brain substance, "miliary abscesses," and melting down of effusions so as to make hyaline bodies alongside of the bloodvessels, often closing their lumen; while the vessels had a pigmented encasement (Pigmenthülle), made up of red blood-corpuscles. These lesions were found in both dogs and men. Dr. Coats published his discoveries in the *Medico-Chirurgical Transactions*, in 1878, accompanied also with plates. His investigations referred to tetanus and hydrophobia. He makes a point of the analogies between these two disorders; and, assuming a virus in hydrophobia, he argues backwards that there is a poison circulating in the blood in tetanus. The lesions he describes in hydrophobia consist of a perivascular infiltration with *white* blood-corpuscles.

In comparing these investigations, it is to be noted that the lesions Coats found in tetanus correspond much more closely with those Benedikt found in hydrophobia than do those the former found in his investigation of this latter disorder. Further, it is to be noted that he describes the perivascular infiltrate as consisting of *white* corpuscles, while Benedikt is clear in describing it as made up of *red* corpuscles. Finally, Coats does not consider (though most persons who refer to him speak as if he did) the lesions he found to be distinctive of hydrophobia, but simply evidence of irritation in the blood. Thus we see that these two investigators cannot be regarded as confirming each other.

Now the association of the name of Dr. Gowers

(whose investigations are described and illustrated in the *Transactions of the Pathological Society of London*, vol. xxviii. 1877) with those of Benedikt and Coats, as having found lesions pathognomonic of hydrophobia, is equally unwarranted, for he distinctly stated, in answer to a question, at the meeting where he demonstrated his specimens (*Lancet*, June 9, 1877, p. 840), that his investigation did not lead him to believe there was any special lesion in hydrophobia.

But, inconclusive and conflicting as the results of all these careful investigations have been, it might be easy to see how they might be taken for more than they are worth, if it were not for the fact that they have been formally and categorically vitiated by the subsequent investigations of others. Thus, Middleton (*Journal of Anatomy and Physiology*, 1881) has published the results of microscopical examinations in which he found perivascular aggregations of round-cells in two cases of hydrophobia, corroborating some of the appearances described by Benedikt, but disagreeing with Coats and Kolesnikoff in essential points. But the most striking point of his publication lies in the fact that he found the same morbid appearances which were present in the cases of hydrophobia in cases of purpura hemorrhagica, diabetes, fracture of the skull, erysipelas, head injury with hæmatoma, concussion of the brain with fracture of the spine, delirium tremens, peritonitis, tubercular meningitis, mental deficiency and excitement, hæmatoma, tetanus, and uræmia. His paper concludes with the distinct avowal that, "as regards the pathology

of hydrophobia, it still remains true that nothing characteristic has been observed."

Another investigator, who has gone over this ground, and come to conclusions as destructive as those of Middleton, is Ivanoff, who says (*London Medical Record*, March 15, 1883, from *Pract*, 1882, No. 15), that appearances similar to those observed by Benedikt, Gowers, and others are found in typhoid fever. One of the supposed characteristics, the transparent spaces, he says, was found by Popoff and Stephanoff in uræmia, and by Vinogradoff in intermittent fever, by Danillo in phosphorus poisoning, and by himself and Dr. Czorkov in the brains of healthy dogs. They have no pathological character whatever.

But I need not detain you with further references to more or less famous investigations. I will only add that they all tend to confirm the opinion that as yet no gross or microscopical lesion has been discovered which can be regarded as pathognomonic of rabies or hydrophobia.

And now I must close. I have not said so much because it is any pleasanter or is likely to be any easier for me than for others to differ from the majority in the branch of science to which my life is given. It is with shrinking that I take a position which, if it is deemed worthy of notice, will be apt to excite opposition, perhaps condemnation. But I have now been engaged for nearly two years in a laborious study of the evidences upon which the general belief in regard to hydrophobia rests, and I have been gradually led to certain convictions different from those with which I started, and which I feel it my

duty to make public. One of these is that hydrophobia is not a specific, inoculable disease, derived from the bite of a similarly affected animal. Another is that if we wish to have an accurate, scientific knowledge of what too often actually takes place, and is usually called hydrophobia, we must begin the study of it unprejudiced by the views heretofore generally entertained in regard to it, and with canons of criticism very different from those heretofore deemed sufficient.

I know that these convictions are not novel, and that they have been stated and defended with arguments before. I know, too, that they have heretofore failed to secure the assent of the greatest men who in every generation have adorned the profession of medicine. None the less, however, do I believe that they are true, and that the time will come when they will not be—as they have been in the past—characterized as too absurd to be discussed, or decried as too dangerous to be tolerated.

But, however that may be, a sense of duty compels me to join myself to the small minority whom I think to be on the right side of the question, for truth's sake. Further, in this matter, the interests of science are in an urgent sense the interests of humanity. For this reason one who feels strongly must speak boldly. This, then, is my apology to those who may think I have erred in opposing the opinion of so many men deservedly honored and respected in our profession.

